

Interactive comment on “Anticorrelated observed and modeled trends in dissolved oceanic oxygen over the last 50 years” by L. Stramma et al.

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Reply: We thank reviewer 1 for the helpful comments. Our reply to the reviewer's comments is following after each reviewers comment.

The manuscript “Anticorrelated observed and modeled trends in dissolved oceanic oxygen over the last 50 year” compared the oxygen trends in the global ocean over the last 50 years from a compilation of dissolved oxygen measurements at 300 dbar with a numerical biogeochemical Earth System model. The paper is well written and relevant in showing how difficult it is to reproduce the observed changes of the ocean properties, in this case oxygen, with a numerical model. Moreover, it is one of a few studies that show changes in the oxygen from observation for the entire global ocean for such a long time scale. The figures support well the analysis although a bigger font could be

C3261

used, and the contour lines in the maps can be made thinner or removed in order to better see the color pattern. There are some major and minor comments that I would like the author to clarify before publication: Answer: Some figures were modified with bigger fonts and thinner contour lines as requested.

Major comments: 1) How the standard error was computed? Did the author account for autocorrelation? It would be good if the authors would describe a bit more in detail about the statistical approach used to compute the errors, and also in case they didn't, to take into account that although the data might be independent the trends can still show, to some extend, serial correlation. If this correlation is not taken into account, the significance of the trend is overestimated and we might see a trend where actually there isn't any. One should consider this when compute the standard error.

Answer: The local trend derived is a weighted least squares solution to the linear system $A \cdot X = b$, with A being a $N \times 2$ matrix which is 1 in the first column and has the date in the second. B is the vector of the observed DO values. The solution is found by determining X through minimizing $(b - A \cdot X)^T W (b - A \cdot X)$ with W being a diagonal matrix of the distance-weights applied. The mean squared error and from that the estimated standard errors can be derived in this process, this is detailed in literature like: Strang, G., Introduction to Applied Mathematics, Wellesley-Cambridge, 1986, p. 398. This reference and a condensed version of this explanation is now included in the revised text. With regard to temporal autocorrelation we had assumed that it is uncorrelated as we could not estimate the correlation.

In the original manuscript, changes in simulated DO were presented as differences between the decade 2001-2010 minus the decade 1958-1967. In order to provide a more consistent comparison with the observational estimate, in the revised version we computed the linear trend in the temporal evolution of DO at each grid point on the 300 dbar surface. For the simulations with climatological wind forcing, this yields virtually identical results, but results differ substantially for the two sensitivity simulations run with interannually varying wind forcing. The standard error of the trend was computed

C3262

directly from the linear regression at each grid point, assuming that all annual mean DO values were statistically independent, and then the global mean trend as well as its error were computed as the spatial average of the linear trend and its standard error (which implicitly assumes that all grid points are statistically independent).

2) Why the choice of using just one single layer at 300 db. Could be that the limitation at this particular layer can cause some of the discrepancy observed with the model simulation?

Answer: The 300 db were chosen as a layer for an observation-model comparison located in the low oxygen layer but at the same time near the surface, as with increasing depth the amount of available oxygen measurements reduces considerably and we like to do the comparison in a region of sufficient data availability to determine oxygen trends on a global scale. We did also a test with the sigma-theta_{26.9} density layer and the results looked very similar. We think that a major factor influencing differences between model and observation are differences in the underlying ocean circulation, whereas the uneven distribution of stations appears to have a small effect. A figure on the distribution of measurements in different decades is now included in the manuscript and a comparison of the model-derived DO trends for complete model output and for model output subsampled at the locations of the observations is included.

3) The authors can explain why they used the WOA 2009 to compute the local DO anomaly and not just use their own data. Is there a risk to increase the uncertainties and/or to detect erroneous changes if the WOA and Hydrobase have different sets of data and been undergone to a different quality control procedure? Maybe the WOA and Hydrobase use the same set of data and have similar quality control. If this is the case, it should be clarified in the manuscript.

Answer: The WOA 2009 maps are used as reference background field since they represent a good estimate of the large-scale DO distribution in the world oceans. Computing our own background fields, would require mapping of smaller scales and higher

C3263

level of quality control, before addressing larger scale trends. By using the WOA a further level of quality control is applied, since strong spatial gradients as observed in absolute DO are reduced to anomalies prior to applying the inter quartile range filter to the local anomaly data. Since there is only a limited amount of DO observations, we assume that the WOA and Hydrobase use largely the same data set. We cannot think of any impact of this on the uncertainties. It is now clarified in the revised manuscript that WOA and Hydrobase use essentially the same DO data set.

Minor comments: 4) The introduction is clear and well structured, however it gives too much emphasis to the OMZs. Since the analysis is global, on my opinion the description of the OMZs can be shortened.

Answer: The information on the OMZs in the introduction was shortened a bit.

5) Was the model validated, if so should be clearly mentioned in the method. Moreover I would like to know how the global oxygen distribution for the layer 300 dbar into the model looks like compared to the observations.

Answer: The model has been validated against nutrient, oxygen, DIC and alkalinity data by Schmittner et al. (2008) and Oschlies et al. (2008). This is now included in the revised version as is a map of the simulated O₂ at 300 dbar.

6) In the section 3 what are the uncertainties of the reported trends?

Answer: The uncertainty of the trend at the station P is now included in the text. The mean trend was computed on the interpolated grid based sometimes on low data coverage (new Figure 1) therefore we did not compute the uncertainty of the mean trend. However for the model runs the uncertainty was computed (see answer to point 1 above) and included in Table 1.

7) Page 4604 line 28 until page 4605 line 1: The sentence is a bit generic since Johnson and Gruber (2007) related the oxygen changes to the NAO for a different time periods and Frölicher et al., (2009) assessed that the NAO account for only 30% of the

C3264

total variability in the subpolar and subtropical North Atlantic.

Answer: The sentence was modified to specify the findings of Johnson and Gruber and Frölicher et al. 2009.

8) Page 4606 line 21 Regarding the data from the 1925 – 1927 Meteor Expedition, are the data quality controlled with the same criteria used for Hydrobase?

Answer: Yes same thresholds applied, this is now stated in the manuscript.

9) Page 4609 from line 3 to the end of the paragraph: How the three effects, solubility, biotic oxygen consumption and changes in transport and pathway processes, are calculated? This could be better explained in the manuscript.

Answer: Solubility changes are computed from simulated temperature (dominant effects) and salinity (minor effects) changes at 300dbar, changes in biotic oxygen consumption are the local changes at the 300dbar surface simulated by the model, and changes in transport is the residual between total changes and the sum of solubility and biotically induced changes. This is described in the revised version both in the text and in the caption of Fig.6.

10) Page 4611 from line 15 to line 20: I don't see an agreement between the simulated oxygen changes with a pCO₂ sensitive C:N ratios and the oxygen changes from the data. The first seems to agree better with the pattern observed in the simulation with constant C:N ratios.

Answer: We agree with the reviewer that the differences between the standard run and the run with variable C:N ratios is not terribly large. In the revised version of the manuscript, this is stated in the text, and the map displaying results of the variable-C:N run is omitted (results are shown for the zonal mean and the globally averaged values in the Table).

Technical comments: Page 4598 lines 23 until 28: The sentence is too long and can be split.

C3265

Answer: The sentence was split into 2 sentences as proposed.

Page 4599 line 5: “an excessive diapycnal mixing” instead of “of excessive diapycnal mixing” Answer: Thank you, the text was modified.

Page 4599 line 14: “mixing intensity” instead of “mixing intensities”. Answer: Text was changed as proposed.

Page 4601 line 17: “smoothed” instead of “smoother” Answer: Text changed as proposed.

Page 4601 line 28: “19” instead of “nineteen”

Answer: “nineteen” replaced by “19” as proposed.

Page 4606 line 5 “: : the Southern Indian Ocean, resulted in periods of decreasing and increasing oxygen trends (Mecking, personal communication, 2012), indicate the trends: : :” instead of “: : the Southern Indian Ocean resulted in periods of decreasing and increasing oxygen trends (Mecking, personal communication, 2012) and indicate the trends: : :” Answer: Modified as proposed.

Page 4606 line 15 “WOCE” instead of “World Ocean Circulation Experiment” Answer: WOCE is now written in the text.

Page 4607 line 6 in the title “Modeled” instead of “Modelled”

Answer: “Modeled” is now written in the title.

Page 4608 line 3 “OMZ” instead of “oxygen minimum zone”

Answer: OMZ is now written instead of “oxygen minimum zone”

Page 4614 line 16: “30_E and 160_E” instead of “30 degrees E and and 160 degrees E” Answer: The reference was modified accordingly.

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C3266